Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

lections of Mr. Charles Fiebig, who lives at Eureka, on Humboldt Bay. As Mr. Fiebig had informed me upon my arrival, that all his birds had been shot in the vicinity of Humboldt Bay, I was surprised and interested to find in his collection specimens of the Yellow Rail, the Emperor Goose, the European Widgeon, and Sabine's Ruffed Grouse, which I saw at once were rather extra-limital species, particularly the first two. Mr. Fiebig's account of these was as follows (transcribed from my note book):

**Porzana noveboracensis.** One specimen 1884, shot on the marsh at the mouth of Freshwater Creek, flowing into Humboldt Bay. A *pair* were seen but only one secured.

**Philacte canagica.** One specimen obtained on the bay in the winter of 1884. Others seen at long intervals.

**Mareca penelope.** One specimen shot on the bay in the winter of 1884. The only record of its occurrence there, according to Mr. Fiebig and the sportsmen of Eureka.

**Bonasa sabinii.** Only one specimen in the collection, but met with on several occasions in the deepest portions of the surrounding redwood forest.

Mr. Fiebig, now a very old man, is a German who came to America about 1850, and worked at his trade of wagon-making, in the city of Washington, until the war broke out, when he enlisted in the Union Army, and afterward settled in California. While living in Washington he became acquainted with North American birds by studying the Smithsonian collections. He is a taxidermist of far more than ordinary ability, but practices the art only as a pastime. His method of mounting is unique; after removing the skin, he carves a model of the bird's body in soft wood, with great exactness. The effigy is then covered with the skin, and the bird, after the eyes are added, is complete. I am bound to say that this is done with the skill of an artist, and the results secured by this method are excellent. Mr. Fiebig gave me an interesting account of his experiences as a Duck hunter on the shores of the Baltic Sea, when a boy.—CHAS. H. TOWNSEND, *Smithsonian Institution, Washington, D. C.*

## CORRESPONDENCE.

[*Correspondents are requested to write briefly and to the point. No attention will be paid to anonymous communications.*]

### The Classification of the Macrochires.

[SECOND LETTER.]

TO THE EDITORS OF THE AUK:—

*Sirs:* If the valuable space in the section reserved for your correspondence will admit of it, I have a few words to say in regard to the criticism passed upon my classification of the Macrochires, so far as I defined

it in my contribution to the 'P. Z. S.' (1885), by Doctor Stejneger in the July number of 'The Auk.'

Dr. Stejneger asks, "Is it possible that Dr. Shufeldt has overlooked the *many* points in which Swallows and Swifts disagree outside of the skeleton?" To this I can answer that I duly took into consideration all of those characters, both internal and external, now generally known to us, when I came to draw up my conclusions in the 'P. Z. S.' memoir, wherein, as the Doctor remarks, the skeletal characters alone appear to have swayed me in my decisions. I am not only conversant with the "*many* points" of difference existing between Swallows and Swifts, but am becoming more convinced every day of the '*many, many* points' of structural difference existing between the Trochili and Cypseli.

Further along in his criticism, when reviewing for my benefit some of the more prominent differences existing between the Swallows and Swifts, as Dr. Stejneger comprehends their structure, he contends that "internally they differ in a great number of points, but we shall only mention that the Swifts have a sternum, while the Swallows have the manubrium bifurcate and the posterior border deeply two-notched." Reading this sentence over carefully a number of times I must confess that its author does not make quite clear to my mind the kind of comparison he wishes to institute between the sternum of a Swift and a Swallow. No one probably will deny "that the Swifts have a sternum," though many might take exception to the remark that that bone was "two-notched" in the Swallows. To be sure it has *a pair* of notches in its xiphoidal extremity; but as generally described, the two-notched sternum is seen in such forms as *Picus*.

Again, Dr. Stejneger asks me in his criticism, "but what differences are there in the Swifts' flight from that of the Swallows' that should have caused such a remarkable modification towards the Humming-birds?" (p. 406). My answer to this question will also constitute a reply to the two succeeding questions of my critic, as it will, I hope, still further impress upon the minds of systematists the warning, already given in my 'P. Z. S.' memoir, that the similarities of certain structures existing between the Cypseli and Trochili are for the most part due to the modification of these structures gradually brought about by the habits or actions of the forms in question. Surely Dr. Stejneger would never have asked the question I have first quoted from him if he had ever had the opportunity to compare in nature the flights of two such birds for example as *Micropus melanoleucus* and *Tachycineta thalassina*. Many a time have I stood in the deep, rocky cañons of New Mexico, and seen one of the former birds pitch down from its dizzy position in the air above, with a velocity that taxed the very eye of the observer to follow, to its nest in the side of the precipice below; check itself suddenly at its entrance; hover for an instant, like a Hummingbird over a flower, with its wings in rapid motion, then enter,—to be gone but a moment,— when it makes its appearance again as if shot from a gun, to be off with the swiftness of the bullet. How different is all this from the flight of the pretty little Violet-green Swallow, with it slight and easy motion, rarely hurried and never precipitous!

Anatomists have long known that in avian forms, such as the Cypseli and Trochili, wherein the powers of flight have through time been brought to great perfection and capable of a high degree of velocity, that it is necessarily accompanied by corresponding modifications of structure, such as a deepening of the sternal keel, and changes "in the shape of the humerus and its processes," as remarked upon by Dr. Stejneger. That the *corresponding* "processes" have become more conspicuous is not to be wondered at when we think for a moment and take into consideration the fact that they probably *have been acted upon by the corresponding muscles involved in the flight.* In other words, when we come to sift out the characters wherein the Cypseli and Trochili *principally* agree we find them to be just such ones as I have elsewhere pointed out, and in each instance are found to be structural characters, the modifications of which are due to similar habits of the forms in question, but this by no means satisfies my mind that the groups should be, or are closely affined. I think sometimes taxonomists too often lose sight of the lines of descent of the class Aves in time, and in their eagerness to show relationship of the remnants of existing forms or groups in recent times, overlook the great gaps that probably exist among the twigs of the branches and stems that represent the tree of their pedigree.

Notwithstanding Dr. Stejneger's warning against placing too much reliance upon the skeletal characters for our guidance, I must still insist that the characters (as we find them in forms which we are comparing) of the skull and axial skeleton are among the most reliable if not *the* most reliable we have. If there be better ones in any vertebrate organization I have not been so fortunate as yet to have met with them. If I find that the number of *vertebræ* constantly differed in any two birds, and their skulls are of a radically different type, why I would no more be swayed from my opinion that they were members of a different order, as orders are regarded in ornithology, than I could be brought to the belief that anatomical characters are valueless in taxonomy. Certainly finding an additional pair of primaries or secondaries in the wing, in either case, would have but little weight towards altering my first opinion, based as I say, on what I had found in the cranium and column. Now to take the skull of a Swift and a Hummingbird as an example, *all*, absolutely *all*, of the leading characters as we find them in the representatives of these two groups are at variance while quite a number of these characters agree in this part of the skeleton with the Swallows and Swifts, and others can easily be shown in the latter birds to be demonstrable departures due, probably, to unknown causes from the typical passerine ones.

Since the appearance of my memoir in the 'P. Z. S.' a considerable amount of material (Macrochires) has come to my hands, thanks to some of the members of the A. O. U. and associate members, and others. A superficial examination of some of this but satisfies me of the correctness of my first conclusions, and if those conclusions are to be modified at all it will be in regard to the Swifts, which I think can be shown to be a group of birds also entitled to a separate order, as orders go in systematic ornithology,

as well as the Trochili and Caprimulgi. This order Cypseli would stand between the Trochili and Passeres, but as I am soon to have my second contribution to this subject in hand, all such questions will therein be considered.

I have a large collection of alcoholics now at my disposal, and am only waiting to secure a better assortment of the Nightjars and Trogons before undertaking the work, or rather pushing it, as many of the drawings are already completed.

In the mean time, permit me to say to those who may be interested in this subject that I deem it quite a possible thing that an offshoot may have taken place from the common stock Passeres, near the Swallows, as would in time have produced our typically modern Swifts. I can picture how these forms at first may have had some change in their environment as demanded an increase of the power of flight. This would demand an increase of the power of the muscles involved therein, and finally we would find just such changes in the bones to which these muscles are attached as we in reality do in existing Cypseli. So that the enlarged pectorals, the deep keel to the sternum and its unnotched xiphoidal extremity, the short humerus of the arm, with its conspicuous processes, are all examples of *physiological adaptations of structure*. So there may have come down to us an entirely different group of birds, as the Hummers, of very different origin, which group may have had the same factor thrown into its environment, somewhere in time, that demanded an increase in the power of flight, and as a consequence we find a similar modification of the parts involved. But when we come to critically examine and compare the modified parts we may find, as we do in the case of the Hummers and Swifts, that although the *same end* has been very prettily arrived at by the changes in the structures, yet at the same time quite *different forms* of the several and corresponding parts had been the result of it all. The first comparison, with the views of pointing out the relationships of such, and existing groups, wherein the fundamental characters are masked by such deceptive similarities, constitute some of the most difficult problems of systematic zoölogy. In the comparisons, it is by no means necessary to eliminate them, but simply we must be guided in our conclusions by what the *sum of all* the morphological characters of the forms under comparison go to show.

It is really no valid reason that we should retain in the same *order*, were vessels so classified, two kinds of them, simply because they might both happen to possess "deep keels" and "short shafts" connecting their wheels with their motive powers, for one of these vessels might be driven by steam and the other by some other force, notwithstanding the fact that one might show an additional blade or two in either of its wheels (wings) or perhaps have a different style of rudder (tail), and yet the fundamenta differences be very great and justify us in widely separating them in any scheme of classification.

In conclusion I must express my satisfaction at finding one who has perhaps thus far devoted his best energies in avian taxonomy to the con-

sideration of external characters and obscure points in synonomy, as Dr. Stejneger has so ably done for us, expressing himself as he does in the criticism of my memoir by saying, "A natural system cannot be based upon one single set of characters; all will have to be carefully considered, whether they are external or internal, before we can hope to understand the true relationship of the different groups" (p. 406). This is precisely, indeed the words are quite the echo of, what I have taught, and my sentiments for a number of years past, as the reader may see by referring to the leading paragraphs in my "Osteology of the Cathartidæ," published in 1883 in Hayden's Twelfth Annual, by the Department of the Interior.

Very respectfully,

*Ft. Wingate, New Mexico,*
4th August, 1886.

R. W. SHUFELDT.

ERRATUM.—In Dr. Shufeldt's letter in the July 'Auk,' p. 414, for "nine," in the first line of the letter, read mine.—EDD.

## NOTES AND NEWS.

THE NEXT meeting of the American Ornithologist's Union will be held in Washington, Tuesday, November 16, and following days. A very interesting meeting is anticipated, and a much larger attendance than usual of both Active and Associate Members is expected. The meeting will be chiefly occupied with the reading and discussion of scientific papers. There will be in addition the usual reports of committees.

DR. F. W. LANGDON, of Cincinnati, has in preparation a work on 'Ohio Valley Birds,' which he hopes to have soon ready for the press. It will be devoted to the birds of the region drained by the Ohio River and its tributaries. Besides some matter relating to ornithology in general, it will include descriptions and life histories of Ohio Valley birds.

A 'MANUAL of North American Birds,' by Mr. Robert Ridgway, is announced as in press, to be published by J. B. Lippincott and Co., of Philadelphia. The work will be abundantly illustrated and, it is needless to say, most carefully and thoroughly prepared.

DR. LEONHARD STEJNEGER announces his intention (Proc. U. S. Nat. Mus., 1886, p. 99) "to write a comprehensive and reliable guide to Japanese ornithology, with ample descriptions of all the known forms from original Japanese specimens." Finding his material for the work still incomplete he earnestly requests assistance in gathering it, in order to enable him to satisfactorily fulfil the task he has undertaken. The work will be based primarily on the Blakiston and Jouy collections of Japanese